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THE TREATMENT OF TYPHOID FEVER BY COLD BATHS.[1]

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Galen defines fever as *calor præter naturam* — preternatural heat. More recent studies have added to, not changed, this definition; but although the mechanism of the preternatural heat has been partly explained by laborious measurements, the real cause is far from being clear. While animal heat was regarded as an inherent attribute of the animal body, just as contractility belongs to muscles or excitability to nerves, the fact of its remaining so nearly constant in the normal mammalian body needed no explanation; but when we consider the body as a complex organism, warmed within by the burning up of part of its tissues, and cooled without by radiation and evaporation into an atmosphere which may vary within limits of much more than a hundred degrees of Fahrenheit, our problem becomes more complicated.

For the sources of heat in the body we have to look to the combustions and decompositions always going on throughout, but chiefly to the vital organs of digestion, secretion, and respiration. The liver, for instance, may be looked upon as one of the most powerful of the furnaces which heat us. The blood is the great distributer of heat, receiving it from the internal organs and distributing it to the cooler surface and extremities. Direct conduction, however, in the trunk at least, plays an important part. The skin is the radiator and one of the evaporators, and the variation in the amount of blood circulating through its vessels, and the greater or less abundance of its secretion, are probably by far the most important circumstances in regulating the withdrawal of heat from the body.

An elevated temperature, as in fever, may arise from either increased production, diminished loss, or both together. It has been generally received until lately, on the ground of an increase of urea excreted in fever as compared with urea excreted by a healthy person on the same diet (that is, a restricted or antiphlogistic), that an increased chemical action throughout the body is the cause of the rise of temperature. A recent writer on fever[1] calls this theory an axiom. This view, however, has been shown to be, if not incorrect, at least only partially representing the truth. The laborious and minute measurements of ingesta, egesta, temperature, and loss of heat, carried out by Senator,[2] show that although the amount of urea discharged by a fever patient is greater than would be discharged by the same patient on the same diet in health, it is not enough in excess of that excreted upon the usual and normal diet to account for the excess of heat produced. The other non-nitrogenous constituents of the body are burnt in even less proportion than those furnishing urea. The amount of heat given off, as measured by him in a chamber carefully constructed for that purpose, although after the beginning of the fever slightly in excess, is not so great as represented by Liebermeister.[3]

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[1] Read at the Annual Meeting of the Massachusetts Medical Society.

It can be shown, on the other hand, that a mere retention of heat, without any increased production, is quite enough to raise the temperature to the febrile point.[4] If *all* the heat usually given off from the surface in half an hour could be retained, the temperature of the body would rise one degree of Centigrade, — or about one degree of Fahrenheit in twenty minutes. A partial retention of heat can be shown, by appropriate methods of measurement, actually to take place, and this too in a way which might easily convey the contrary impression, that is, that the body was growing cooler instead of warmer. In the so-called cold stage of intermittent, when the extremities and skin are shrunken and cool, the patient shivering and complaining of cold, the temperature of the interior of the body is actually rising, and for the very reason that the exterior of the body is poorly supplied with blood, and has consequently become an imperfect radiator. The case is precisely similar to that of a house warmed by steam pipes, where the boiler and rooms near it can be made hotter by shutting off the steam from the more distant radiators. Later, the arterioles relax, the warm blood flows quickly through the skin, is exposed to the cooler air, the sweat flows freely, is evaporated from the surface, and the body is cooled in accordance with well-known laws of physics.

This tendency to return to the normal temperature is a result of many and complicated relations between circulation, secretion, and the sensitive and vaso-motor nerves, relations which may be interrupted by certain lesions, such as section of the spinal cord, and which are probably disarranged but not interrupted in fever. In the former case the mammalian becomes more or less a cold-blooded animal, and his temperature depends upon that of the surrounding atmosphere, of which his heat-regulating apparatus no longer makes him comparatively independent. This is not the case in fever. The fevered organism is not incapable of regulating its temperature. It does not follow that of the surrounding atmosphere, but it does regulate itself at a higher point than normal. The apparatus is readjusted for 103° to 106° instead of 98°, and this temperature has just as unfavorable a result upon the activity of the heart and the constitution of the blood as if it were the result of artificial or extraneous heating.

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[1] Dr. H. C. Wood. A Study of the Nature and Mechanism of Fever. Fourth Toner Lecture. 1875.

[2] Untersuchungen über den Fieberhaften Process und seine Behandlung.

[3] Volkmann's klinische Vorträge. No. 19.

[4] Winkonitz. Wiener Klinik. Jahrgang I.

That increased temperature is really the cause of many of the phenomena of fever can hardly be doubted. Many of them can be reproduced by artificial heating, and disappear on withdrawing the source of heat. The heart beats more rapidly when its temperature is raised a few degrees, but is also more easily exhausted. Urea is formed in greater abundance, but instead of coming in part from the food it is a derivative of already formed tissues, especially of muscle and blood. As regards the destruction of nerve-tissue, we know less physiologically ; but in long-continued cases few of us will doubt, on clinical grounds, that it suffers severely. The fever-heat increases combustion, and increased combustion augments the heat.

In typhoid, the mechanism by which heat is accumulated cannot be so clearly made out as in the short and simple paroxysm of intermittent. If the skin were dry and anæmic throughout the disease, we should have the conditions necessary for a constant retention and heaping up of heat; but we know that the contrary is often the case. It is probable, however, that only during a small portion of the time in typhoid is the skin in a fitting condition to carry off the maximum of heat necessary to completely regulate the body-temperature, and that although it can and does, at the height of the fever, radiate a larger amount of heat than in health, yet it is not capable of disposing of all which is produced by increased combustion. If we recognize the fact that excessive febrile temperature, as increasing organic combustion, as perverting the action of the nervous system, and especially as exhausting and weakening the heart, if not the only injurious phenomenon of typhoid, is at any rate one of the most so, and in a certain proportion of cases the only cause of death, then the permanent or temporary reduction of that heat becomes the principal object to which a rational treatment should be directed. For this purpose two courses are theoretically open: the diminution of heat-production, the acceleration of heat-loss. Neither of these is impossible for a short time, neither is possible without continuous treatment. Heat-production may be limited by drugs which retard metamorphosis, especially abnormal metamorphosis, of tissue. The most important and efficient of these is quinia in large doses, which is known not only to produce in physiological experiments the effect just stated, but which can easily be shown to reduce a morbid temperature, even in continued fevers, for many hours. This is no unimportant adjunct of the treatment we have specially to consider.

But it is the other factor, which naturally varies within the widest limits, which is capable of fully compensating an increased heat-production, and which is also easily under our control. It has been estimated by measurements of the amount of heat given off from various parts of the surface of the body, that an entirely possible loss of heat could, if continuous, compensate more than three times the normal heat-production, and that the variations in the amount of heat given off are quite enough to account for the constancy of bodily temperature under all circumstances of climate, without any reference to a change in heat-production. A retention of heat in the body during the initial stages of fevers can be demonstrated. For a paroxysm of intermittent or a short attack of febricula, the amount retained during the initial stage, when the limbs and the surface are cold, is sufficient to account for the whole rise of temperature which is taking place from the beginning. In continued fevers there is more difficulty in explaining why the temperature continues so long at an abnormal height, although it is probable that subsequent accumulations of heat of the same kind, arising from slighter chills which may occur in the course of the disease, are added to the initial retention. This retention is brought about by the changes in the circulation of the skin. An anæmic and marbled surface, even if warm, radiates much less heat and cools much less blood than one in which the circulation is active; while, on the other hand, a surface in which venous stasis has taken place, even in slight degree, parts also with little heat. While the amount radiated from a surface which has been briskly rubbed or reddened with mustard-water is increased, it is diminished if strong irritation causes even the slightest approach to passive congestion.

Exposure of the skin to cold, by contracting the superficial vessels, also diminishes its ability to radiate heat, and this diminution may take place from cold air, which is a bad conductor, as well as from cold water, which is a good one; so that we can see why exposure to a cool atmosphere is not so efficient in removing heat as immersion in cold water.

It has been stated by at least one experimenter,[1] and he too one of the warmest advocates of this method, that more heat is produced when the surface is cooled, and that the real efficacy of the baths is owing to a subsequent reaction from the over-production, and not to a primary cooling. This, however, cannot be looked upon as proved, and the increased elimination of carbonic acid, which is one of the supports of this theory, is possibly only excretion, and not formation.

The action of a cold bath is probably somewhat as follows: The external surface is more or less rapidly chilled. Less blood circulates therein, and the skin and subcutaneous tissue become bad conductors, thus attempting to protect the body against the abstraction of heat. It is physiologically possible, as we see in the case of the marine mammalia, that an animal should be so thoroughly protected by his skin and fat as to be capable of resisting indefinitely, in the water, a temperature far below that of the body; but in man, whose heat-regulating apparatus is arranged for another surrounding medium, and who is in the habit of supplementing the protection given by his skin with artificial coverings, this is only possible for a short time, and then the temperature of the body in general must, after a longer or shorter time, fall and reach that of the surrounding medium; that is, the protection is temporary and imperfect.

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[1] Liebermeister. Loc. cit.

In an ordinary bath of from ten to twenty or more minutes, the fall of temperature is not much felt at first in the interior of the body. In the limbs, which present more surface in proportion to their bulk, and which form but little heat for themselves, the cooling action is much more felt; and the inequality of temperature between different parts of the body often gives rise to the feeling of chill, which, although of course not agreeable to the patient and somewhat alarming to those practicing this method for the first time, has absolutely no unfavorable import or result. When the patient is removed from the bath, the external cold being replaced by warmth, the arterioles dilate, the blood circulates more rapidly through the cooler external layers, and the abnormally high temperature of the interior is reduced by the transfer of heat to the surface to nearly, but not quite, its former height. Thus it is found that temperature continues to fall for some time after a bath, and does not reach its lowest point when the bath is over, but half an hour or so afterward. The temperature after a time again begins to rise, and attains its former height in a longer or shorter time, according to the intensity of the disease.

After considering the facts just stated, and especially certain important points which are yet far from being settled, such as the alleged over-production of heat during a bath, it will easily be granted that although physiology and experiment may lead us to look favorably upon the cold-water treatment, and furnish a sort of *a priori* argument in its support, yet the final judgment must come from actual experience. Cold bathing in typhoid is no new thing, having been proposed and used as a remedy in the latter part of the last century by Jackson and Currie,[1] and with considerable reputation; but it soon fell into neglect, possibly for the same reasons which will oppose its spread at the present time.

Various methods of applying cold externally in typhoid fever have been used, but of these some demand no more than a mere mention. The cold affusion, cold shower-bath, or a plunge into the sea are not likely to find general acceptance, and observations as to their effects will be rare.

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[1] Medical Reports on the Effects of Water, cold and warm, as a Remedy in Fever and other Diseases. Philadelphia: 1808. (From fourth London Edition.)

We have really to consider four: —

1. Cold sponging, more or less frequently repeated, is the easiest, but the least effectual, in withdrawal of heat, and it is chiefly of use when other means cannot be employed. Its soothing influence upon the dry and burning skin is, however, of no small advantage.

2. The cold pack is a method which may be of use in private practice, if the others are objected to. Two or three renewals of the wet sheets are necessary, however, to equal in effect a full bath. It has been suggested that the sheets may be repeatedly sprinkled with cold water, and of course if this is frequently and thoroughly done, the method approximates more or less a full bath.

3. The simple full cold bath by immersion is the method which has been most employed, and which is perhaps the most efficient, demanding a less expenditure of time, labor, and water. In Germany, apparently, the same tub of water is used several times. Against this method is chiefly to be urged its apparent harshness.

4. The fourth plan is the one which seems to me to combine greater advantages than any other, although it is, to be sure, the only one of which I have personal experience, unless the first be excepted. With the arrangements in our hospitals and in the better class of dwelling-houses, it is not difficult to carry out. The patient is placed in a bath near the temperature of the body, 100° for instance; then warm water is drawn off and cold added until a limit of between seventy and eighty degrees or less is reached. Two or three feet of India-rubber hose slipped upon the cold-water faucet is convenient in cooling the bath equally, and avoiding the splash and noise of the running water. The patient's limbs should be rubbed by attendants to equalize as far as possible the circulation, and thus promote the cooling from a larger surface. After ten or fifteen or more minutes he may begin to shiver, and he should then be placed in bed and covered up. If the chill be severe, heaters may be placed to the feet, the amount of heat thus added at a distance from the trunk being small, while it greatly relieves the subjective sensations of chill. A glass of wine may also be administered, especially if, as sometimes happens, the pulse becomes small. The effect of the latter is to excite the heart and dilate the arterioles of the surface, thus promoting the transfer of heat from the interior to the exterior. Its physiological action seems to be peculiarly adapted to this stage of the treatment, and, indeed, I am not sure that it would not be better to make the wine the rule rather than the exception.

The disadvantages of this form are the necessary time, labor, and water, disadvantages which, of course, must vary greatly in weight according to the circumstances of the special case. It is also somewhat, though not materially, less efficient than the full cold bath, and if long continued its effects are possibly of longer duration. In its favor are its great apparent mildness, and, possibly, a less tendency to reaction and over-formation of heat.

How often and when should baths be given? The German authors answer this question very simply by saying, Whenever the temperature, which should be measured at least as often as every two or three hours, reaches a certain point. This point is fixed by Liebermeister at 102° if taken in the axilla, or 103° in the rectum; by Ziemssen[1] at 104° in the rectum during the first two weeks of the fever, later, at a somewhat lower point.

Such frequent measurements, and especially carrying on the bathing through the night, demand an amount of skilled labor which has not been at my command, and my habit has been, upon observing the temperature of the day before, and the condition of the patient at the morning visit, to order the baths for the day, usually specifying approximately the hours.

Ziemssen states that when gradually cooled baths are used, and one is given whenever the temperature reaches 104°, at first, usually four or five, or at the most six, baths are necessary in the twenty-four hours, and that these usually fall between the hours of one and two, six and eight, and ten and eleven in the forenoon, one and two, four and six, and eight and nine in the evening. When less than six are given, that between eight and nine is first omitted, and then one in regular succession from the beginning. Four baths per diem I have often given, but never, I think, six.

With four I have usually two in the afternoon, to anticipate the evening rise of temperature, and one as late in the evening as is compatible with the arrangements for night-watchmen, etc., in order to have the effect last as far into the night as possible. A bath in the evening often helps materially in procuring a good night's rest. I admit that this is only an imperfect carrying out of the system, but the circumstances I have alluded to rather than my own wishes are responsible therefor.

Let us consider what practical experience says of the advantages of the cold water method, and what it can or cannot do. I now speak of the more efficient methods, that is, the cold or gradually cooled full bath, and most distinctly do not mean in the least tepid or cold sponging, although we may be obliged to use it as an inefficient substitute. In using statistics, which I shall do sparingly, I am fully aware how erroneous may be their teaching, from the varying intensity of different epidemics, the class of population from which the patients are drawn, the time at which they seek assistance, the treatment other than medical, the hygienic surroundings, and the varying diagnosis. But sufficiently large numbers may now be obtained to eliminate some of these sources of error, and among them series of cases from the same hospitals, where two different methods have been practiced.

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[1] Ziemssen and Immerman. Die Kaltwasser Behandlung des Typhus abdominalis. Leipzig. 1870.

As there may be those who will attribute a decreased mortality under cold water rather to a disuse of other treatment than to a use of the baths, and will intimate that the patients get well not because they are *bathed*, but because they are *not drugged*, we ought to take as our standard of comparison series of cases which have received but little active treatment, and inquire whether this method produces any favorable change in the natural course of the disease. Unfortunately the large numbers of cases assembled by Murchison cannot be taken as representing a purely expectant treatment, but merely an average of all treatments, into which we may suppose the expectant entered pretty largely. The more modern the statistics, the fewer disturbing elements, we may suppose, will affect the natural course of the disease. This abstinence from active medication is probably on the whole a gain, but it by no means follows that all plans of treatment must have been harmful, nor does it follow that a pure expectancy is the safest treatment.

It is to be remembered that none of the modern promoters claim for cold bathing any power to cut short the typical and, so to speak, normal course of the disease, nor to rob it of many of its essential features. The fever, that is, the abnormally high temperature, is the only symptom directly affected thereby; but as a secondary consequence of this we have, as is claimed, slower pulse and stronger heart, less nervous disturbance, less delirium, less diarrhœa, fewer bedsores, and a more rapid convalescence. In this way the length of the sickness may be shortened, though the "run of the fever" is not, and the favorable results become most evident in a lighter form of disease and a decreased rate of mortality. We do not so much expect wonderful recoveries in very severe cases, as we do large numbers of cases which, if this treatment be early adopted, do not become severe at all and do not develop in the highest degree the most dangerous symptoms of the disease.

An average rate of mortality from typhoid might be stated which would be accurate within quite narrow limits, but the value of such a statement for our purpose would be less than that of statistics giving more fully the methods of treatment.

In the eighteen thousand cases tabulated by Murchison the average mortality was eighteen and sixty-two hundredths per cent., but the separate groups of cases which went to make up this large number varied from sixteen to thirty-two per cent. Chambers gives one hundred and twenty-one cases, with a mortality of only two and a half per cent.;

three hundred and three cases from the Massachusetts General Hospital give a mortality of about thirteen per cent.; Dr. Upham's one hundred and fifty-two from the City Hospital,[1] about fourteen per cent.; Dr. Flint's seventy-three cases, about twenty-four per cent. In two sets of cases at Bellevue Hospital, quoted by Dr. Flint, in which the treatment was chiefly by stimulants, except that in one series dilute sulphuric acid was administered, the rates were respectively twenty and ten per cent.

M. Bordier[2] represents the average mortality in the hospitals of Paris under the ordinary methods of treatment as from five to nine per cent.; but in the one hundred and forty-seven Parisian cases included in Murchison's collection it was thirty-two per cent.

Now as to the results from cold bathing: Jürgensen[3] reports from the same hospital at Kiel a reduction of mortality from fifteen to three per cent.; Liebermeister, from sixteen to nine per cent.; Ziemssen and Immermann, from thirty to nine per cent. Brand reports fourteen hundred and eleven cases with a mortality of four per cent., and one series of one hundred and seventy with no mortality at all.[4] Lissauer[5] observed in three army hospitals a mortality of twenty-four per cent. under expectant treatment, six by cold water. His numbers are, however, small.

M. Bordier, in criticising these with some other statistics, says that the wonder is not so much why patients have done so well with the baths, but why they did so badly before, and endeavors to show from his table (which is a very complete one) that the average results claimed by the advocates of cold baths are really no better than those obtained by others who do not use them; but in his own tables, the total calculation of which is of no great value, since the number of cases upon which the percentages are computed is not given, we find the groups where the mortality is zero only upon the cold water side, and of these there are six, not including Brand's which I have just mentioned.

Giving due weight to the fact that many physicians who have published tabular statements on this subject are ardent advocates of this treatment, it certainly seems as if the comparison of series of cases in the same hospital were as little open to objection as any method of using statistics. It would be easy to collect many more groups of this character even from the table of M. Bordier himself, but as there are but few statistics directed to this point which give a different result, I may refer at once to these exceptions.

Two series of cases are reported from Duchek's clinic at Vienna where the cold water treatment gave a mortality of twenty-eight per cent. as against twenty-seven per cent. without such treatment.[1] The figures are too high and the difference too small for us not to suspect that something more than either baths or the omission of them must have been responsible for this great mortality of more than one death in four patients.

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[1] Medical and Surgical Reports. Boston City Hospital. First Series.

[2] Journal de Thérapeutique. 1874, page 465, *et seq.*

[3] Klinische studien über die Behandlung des abdominalis Typhus mittelst des Kalten wassers. Leipzig. 1866.

[4] Ziemssen and Immermann. Op. cit.

[5] Centralblatt für die medicinische Wissenschaften. 1872, page 47.

Four other series give in typhoid fifteen per cent. for bathing as against twenty-eight for expectancy, but in typhus the results were against the cold water by twenty-six to eighteen.[2]

I think it will be granted from the statistics given above, or from many others which might be added, that cold water gives at least as good results as the very best to be attained by expectancy or any other treatment. To me it seems much better.

Of my own experience I speak with some diffidence, although it has had no small share in forming my own opinion, and I am so firmly convinced of the great benefit to be derived from this treatment that I sought an opportunity to read this paper for the purpose of commending it, and am willing to be considered its advocate rather than its critic. But in the first place the numbers I can present are too small to be of much value alone; secondly, I have been unable in all cases to carry out this plan so thoroughly as I could wish, especially at night; and thirdly, the figures alone, taken without explanation, furnish no very strong argument for the conclusions I draw. Taking the cases of typhoid fever which have been under my care in the City Hospital during three years previous to the present, except a few which entered convalescent or nearly so, or in which the result may be considered to have been practically decided before they fell into my hands, I reckon sixty-six in all, from which a stricter scrutiny might throw out more than one. These will not absolutely correspond with the diagnoses made in the indices to the hospital books, where typhoid fever covers many cases entering in the typhoid condition, where the interest and affection of friends is not sufficiently great to induce them to furnish any information as to previous symptoms, but becomes strongly developed when an autopsy is asked for. In these sixty-six cases, most of which when it was required were treated by more or less bathing, eleven were fatal, the death-rate thus being about eighteen per cent.

It is evident, however, that the results of a treatment which aims to control symptoms by mitigating the general violence of the disease will be better shown where it is pursued from an early period, than in a promiscuous collection of cases subjected to it at all stages of progress; and consequently a series from the first week of the disease is a fairer test than the average of all. In the table of typhoid fever at the City Hospital, given by Dr. Upham,[3] I find forty-six cases entering in the first week of the fever, with five deaths, or a little over ten per cent. Of my own, as I have classified them, I have in the three years (1872–74) thirty-four such cases, with a mortality of three; but if the beginning of the disease is reckoned as Dr. Upham has done in his table, that is, from the occurrence of certain severe symptoms and not from taking to the bed, one case and one death would be counted out, giving a mortality of about six per cent. But of these two deaths one was of a child, where there were really no clearly diagnostic symptoms of typhoid, fever and abdominal tenderness being alone noted, and the absence of diarrhœa specially mentioned. No autopsy was made. The remaining case presented an unusual intensity and character of nervous symptoms, and the fatal result was due to sloughing and perforation in the sigmoid flexure of the large intestine. I think, therefore, I am fully justified in stating that of thirty-two patients in three different years, where a clear diagnosis of typhoid fever in the first week is admissible, only one died.

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[1] Centralblatt für die medicinische Wissenschaften. 1871, page 336.

[2] Centralblatt für die medicinische Wissenschaften. 1872, page 383.

[3] Medical and Surgical Reports. Boston City Hospital. First Series.

During the present year I have had two well-marked cases. One, a man, was bathed, although at first not liking it very well, and is now convalescent; the second, a woman whose nervous system was from the beginning severely affected, objected so strongly to the baths that it was almost impossible to carry the plan out efficiently, the water never being enough cooled to materially reduce her temperature, and they were accordingly abandoned. She died, apparently of the fever, pure and simple, on the twenty-third or twenty-fourth day. There were no signs of any of the accidents of the disease except the pulmonary.

Two obstacles have stood and will still stand in the way of this treatment, especially in private practice; they ought not, however, to be insurmountable. The first is the objection raised by the patient and his friends. The fatigue and apparent exposure and harshness appear somewhat repulsive at a time when, as now, anything like decided or positive treatment in acute diseases is so likely to be looked upon as old-fashioned and unscientific. In a certain proportion of cases, not, I think, very large, the patient himself objects decidedly to the baths, — less however to the fatigue than to the chilly sensations. Persuasion, custom, and experience of the relief afforded will often overcome this; but when, as will once in a while happen, and as did happen in the case spoken of a moment ago, the repugnance cannot be overcome, the physician must, as with other powerful remedies, balance the good and the harm against each other and act accordingly. In a great many cases, however, patients enjoy the baths, and await the time prescribed with impatience.

Another most serious obstacle is the increased time, labor, and attention called for on the part of the attendant. It is much easier to pour into the mouth of a patient at stated intervals a teaspoonful of liquor ammoniæ acetatis, or something less innocent, than to give a bath, which cannot be done for an adult without two tolerably strong and skillful persons. I have however, at the hospital, met with much less complaint of the additional labor than, I confess, I expected. Where the ordinary bath-tub, with its hot and cold water, is wanting, the difficulties are of course increased, but are hardly insurmountable except in the dwellings of the very poor, whose severely sick typhoid patients (in the city at least) should never be treated at home. Ice might be of good service in this way; a piece being put into the warm bath would produce even greater regularity of cooling than we get from turning in cold water. The cold pack, although laborious and comparatively inefficient, calls for nothing which cannot be found in any but the very poorest houses where, as I said, typhoid should not be treated. If the wet sheets are re-watered, as suggested by Dr. Flint, the effect will be greater and last longer. Here again, perhaps, ice might be used to advantage.

Finally, if no other method can be used, cold sponging, although the feeblest of all the four, is better than nothing, and should be frequently repeated. It may be done with water, water and alcohol, which by its evaporation produces more cooling, or, as Jaccoud recommends, aromatic vinegar.

It is of course to be understood that this treatment does not exclude other means, which may be useful or necessary either in the way of regimen or medicine. The importance of the most thorough ventilation, cleanliness, and disinfection is not in the least diminished. The rest of the treatment may be the purest expectancy, the symptoms as they arise may be met by appropriate means, or the antipyretic action of the baths may be supplemented by certain drugs. Digitalis and quinine have both been used. Of the former, for this purpose, I have no experience, but quinine I have several times used after the manner of Liebermeister and others, that is, not in the so-called tonic doses frequently employed, but twenty, twenty-five, or thirty grains in two doses near together. The temperature is lowered for several hours, and fewer baths may be used. The antipyretic action of alcohol, so far as it goes, is in its favor, but its power in therapeutics is but slight in this direction. As an adjunct to bathing or as taking the place of food, either with or without bathing, the opinion of the profession is largely in its favor.

My allotted time has permitted but a brief sketch of this method and the principles upon which it is founded. That it has been used and its value recognized before our time is far from being an argument against its use, since we seek for truth and not for novelty. That it has since been neglected is fully accounted for by the practical difficulties already described, and by an inefficient use of it. The worst that can be said of it is that it is unnecessary and gives no better results than letting patients alone; the best is that it is one of the most efficient means of treatment in the whole long catalogue of therapeutic agencies.

OVARIOTOMY.

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II. In April, 1874, Mrs. X., thirty-two years old, came under my treatment for an abdominal enlargement of four years' duration. I made a doubtful diagnosis of an ovarian cyst, but advised against an operation for its removal, in deference to the humane dictum of Spencer Wells, that we have no right to subject a patient to the risk of such a measure unti either her sufferings are so great as to render life no longer desired, or the rapid failure of health and strength makes it evident that longer delay will render speedy death inevitable, even without an operation. It is also undoubtedly true, as maintained by the same writer, that the chances of a successful operation are greater after the tumor has reached such a size as to press forcibly on the surrounding parts, whereby with every movement the peritoneum is exposed to a friction that gradually modifies its texture, rendering it callous and less prone to take on inflammatory action.

I saw my patient from time to time during the year succeeding the first interview, and by treating various symptoms as they appeared was able to give her comparative comfort during that period.

Toward the end of February, 1875, Mrs. X. told me her pain was so constant and harassing that she must have relief at any cost. She was at times much more distended than at others, and was often kept awake all night by distressing dyspnœa. Her abdomen now measured forty-six inches at the umbilicus. The symptoms were evidently due in great measure to ascitic fluid, which, together with very fat abdominal walls, masked the nature of the tumor. Still more doubt was thrown on the diagnosis by the history of an attack of jaundice and "liver complaint" just prior to the discovery of the tumor, and by the presence of menorrhagia for several years. Examination showed pretty conclusively that the liver was not the seat of the growth, and a fibroid of the uterus was at length excluded by means of a procedure which I have reason to think is new. The uterus was found to be retroverted, the cervix movable, but the fundus held firmly. The tumor could not be reached per vaginam, and no direct impulse or wave of fluctuation could be transmitted from the epigastric region to the uterus owing to the amount of ascitic fluid intervening between the abdominal walls and the uppermost part of the tumor. The uterine sound was of no avail. In this dilemma I placed the patient in the knee-elbow posture, with the idea that if the tumor were a fibroid its weight would cause it to sink in the ascitic fluid until it rested on the abdominal walls; in this position I should be able to obtain by palpation some information as to the character of its surface, could at the same time send a direct forcible impulse to the uterus, but, more especially, could derive some indications from the change in the shape of the vagina and its relations to the cervix uteri. I argued that if the tumor were a fibroid, continous with or closely attached to the uterus, its weight, dragging upon the vagina by means of the cervix, would cause the vagina to be drawn out into a long funnel, at the end of which would be the cervix, not projecting as before, but so retracted as to be almost level with the surface of the vagina. A thin-walled ovarian cyst, on the other hand, would be scarcely heavier than the ascitic fluid; it would consequently not change its position materially on change of the patient's posture, its slight amount of traction would not bear upon the uterus alone, but would be distributed throughout the broad ligament, from which its pedicle would chiefly spring; as a consequence of these conditions, the vagina would be less distorted and the prominence of the cervix be less modified; in addition, the impulse from above would not be forcibly transmitted to the uterus, and the surface of the tumor would not become accessible to palpation. Two contingencies might nullify the inferences to be drawn from these indications: the presence of pelvic adhesions, and a partially solid character of an ovarian tumor. But adhesions are known to be rare with fibroid tumors, and, as far as my experience goes, the more solid portion of multilocular ovarian cysts is generally situated in the pelvis and easily accessible. This method is of course only applicable to small and medium-sized tumors surrounded by considerable ascitic fluid. Governed by the above considerations I diagnosticated an ovarian cyst, probably unilocular, free from adhesions, and with no solid part, but surrounded by a varying amount of ascitic fluid.

The tumor was removed on March 6, 1875, with the assistance of Drs. G. H. Lyman and J. N. Borland, in presence of Drs. C. E. Buckingham and J. P. Reynolds. Ether was administered by Mr. Fletcher Abbott. The same extreme care to avert the possibility of infection and to secure every hygienic advantage was observed in the preparation for the operation as was detailed in the report of my last case.[1]

The correctness of the diagnosis was verified; there was less ascitic fluid than was expected, but I had been seeking to promote its absorption for several days by the administration of a saturated solution of chlorate of potash. The abdominal walls were fully three inches thick; an incision four inches long was made in the median line; the pedicle was very short, but a Wells clamp was applied without difficulty, and the incision was closed with silk sutures. It was found impossible to cleanse the peritoneal cavity completely, for a pinkish serum was seen to ooze from every part of the peritoneum as fast as it was wiped away. Two gallons of fluid were in the cyst. The total weight of the tumor was eighteen pounds.

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[1] Boston Medical and Surgical Journal, xcii. 397, April 8, 1875.

The recovery from ether was speedy and unattended by vomiting. Morphine to the extent of a grain was required to relieve the pain apparently due to the traction on the pedicle. Three hours after the operation the patient was dozing, so that I ventured to leave her in charge of the nurse for half an hour. On my return, however, I found her in a state of profound collapse, almost pulseless, scarcely breathing, blue, and clammy. Brandy and heat had already proved ineffectual to arouse the vital forces. I immediately removed the dressings from the wound and, finding the abdomen considerably distended, took out nearly all the sutures, broke up the agglutinations, and allowed much serum to escape; on search no effusion of blood was discovered in the peritoneal cavity. The patient revived at once. The pedicle seemed so tense that, before reclosing the incision, I tied it firmly below the clamp, removed this, and dropped the pedicle in Douglas's pouch. A similar sudden revival from imminent death was observed in 1867 by Koeberlé, who, during an acute peritonitis subsequent to ovariotomy, made an incision into the right flank, where dullness was detected, and allowed a large accumulation of pinkish serum to escape. The patient rallied at once and made a good recovery.

During the three following days the patient's condition, if not altogether satisfactory, was certainly not alarming. She was kept on a light diet for thirty-six hours, and then stimulated moderately. On the second day there was considerable vomiting, with slight tenderness and distention of the abdomen; she was very fretful and intolerant of restraint, kept her legs in constant motion and frequently shifted her body about on the bed. The pulse ranged from 110 to 130, and the temperature from 99° to 102° F. In the afternoon of the fourth day the pulse fell to 110 and the temperature to 100.5°; enemata had at length brought flatus from the rectum, the tongue had become moist, and the wound began to suppurate in a healthy way. Toward night, however, without any increase in the abdominal tenderness or other indication of peritonitis, the symptoms became ominous. On the next morning, the abdominal distention was so great as seriously to impede respiration; no wind escaped either by mouth or by rectum. I summoned Dr. J. J. Putnam to try the efficacy of galvanism in exciting peristaltic action, but his efforts were unavailing, and I was obliged to puncture the intestines, through the abdominal walls, with a fine trocar. Much gas escaped through the canula, with immense relief to the respiration. The patient continued to sink, however, in spite of constant stimulation by means of brandy and beef-tea enemata, brandy and a solution of quinine subcutaneously, and she died on the fifth day.

A complete autopsy was not made, but Dr. E. G. Cutler examined the peritoneal cavity in my presence. We found evidences of recent peritonitis, limited, however, to the neighborhood of the abdominal incision; the peritoneum above the umbilicus was perfectly healthy in appearance, as was that lining the pelvis and the pelvic organs; it was evident that no inflammation had spread from the cut end of the pedicle. The uterus and appendages were removed. The kidneys, liver, and spleen were healthy.

In view of the mildness of the symptoms after the operation, and the limited extent of the peritonitic inflammation found at the autopsy, I was at a loss to explain the cause of death in one so well-nourished and of so good constitution as my patient appeared to be. A week after the patient's death, however, it was reported to me on unquestionable authority, that for three years she had been in the habit of a free indulgence in spirituous liquors, frequently imbibing from one to three pints of rum in a day, though never intoxicated. This practice had not only been kept from my knowledge, but was absolutely denied on the day preceding the operation. Some nervous symptoms had aroused my suspicions, and led to my putting some categorical questions as to the amount of stimulants consumed daily; I explained the importance of my knowing the truth, as the after-treatment always had to be adapted to the habits of each individual. She disclaimed emphatically ever drinking anything but a pint of beer a day; my suspicions were consequently set at rest. Had I known the facts of the case I should have declined to operate. As it was, there was no apparent reason before the operation why the result should not have been as favorable as in my last case.

I have now no hesitation in ascribing the fatal result to exhaustion immediately dependent upon the physical condition engendered by the immoderate use of alcoholic stimulants. All surgeons know how full of risk even trivial operations are apt to be in individuals who are saturated with alcohol.

In connection with the puncturing of the intestines with the aspirator to relieve the flatulence, it seems to me that another indication may be met by injecting brandy, beef-tea, etc., through the canula into the small or large intestine, after the gas has escaped. By this means a patient's strength might be sustained when nothing could be retained in the stomach, and absorption by the rectum was too slow to meet the demands of the system. In this way fluids could be introduced in considerable quantities into that part of the alimentary canal from which they would be most readily absorbed. If the use of the trocar is as harmless as is asserted by those who have tried it, I see no reason why the intestines should not be repeatedly tapped for the injection of nutrient and stimulant fluids in many desperate conditions and diseases, when the other resources of medical art have failed. In such a case as mine I believe that there would have been no other means of introducing enough alcohol to carry her through the depressing effect of such an operation, had her habits been made known to me during life. Might not peristaltic action also have been excited by the same measure, and thus a second advantage been derived from this procedure? In case of fæcal impaction, may not the hard, scybalous masses be softened and broken up by the injection of different fluids into their midst, when enemata and purges have proved powerless?

Report upon the Specimens by Dr. Fitz. — "The tumor consisted of a single sack intimately connected with an elongated, dense, flattened mass, shaped like the normal ovary but certainly six times as large. This body was composed of a fibrous stroma in which were several small cysts, whose general appearance was that of Graafian vesicles (follicular cysts). The large cyst was contained within a thin capsule of firm fibrous tissue (penitoneum), to which it was loosely connected by areolar tissue. In this intervening layer a cyst of the size of a bean was found, which was lined with ciliated epithelium, the ciliary movement still going on. Between the outer investment and the wall of the cyst, the Fallopian tube was discovered; it was nearly a foot in length, was of normal size where it had been cut in the pedicle, but farther on was dilated; it completely encircled the tumor and terminated blindly near the ovary, being lost in the sheath of the cyst. Fimbriæ were not observed. The proper wall of the cyst was smooth, thick, of grayish color, thrown into numerous folds, moderately vascular, and lined with a cylindrical non-ciliated epithelium. The fluid from the cyst contained albumen. There was no evidence of secondary cysts. The structure of the tumor, and its relation to the ovary, the Fallopian tube, and the investing membrane (containing a cyst lined with ciliated epithelium), from which latter it could be readily enucleated, render it probable that the tumor was an ovarian cyst which had grown between the layers of the broad ligament.

"The left ovary was of normal size, and contained several small follicular cysts. A unilocular cyst as large as a walnut (which at the operation was taken for dropsy of the tube) extended outwards from the left end of the ovary, between the layers of the broad ligament. It contained a yellow, gelatinous fluid, in which were numerous granular corpuscles. The wall of the cyst was composed of fibrous tissue in which were numerous concretions, often microscopic. One as large as a grape-seed projected from that part of the wall which was directly connected with the ovary. The wall was lined with tesselated epithelium. The albugineous tunic of the ovary was lost in the wall of the cyst."

In spite of the fact that the histological elements above described would be sufficient, according to pathologists, to include these two cysts among the ovarian, yet I cannot but feel that the extreme improbability of two ovarian cysts in the same patient, developing between the layers of the broad ligament and not in the common direction toward the periphery of the organ, casts some doubt upon the correctness of the commonly accepted diagnostic points of ovarian and parovarian cysts. Dr. Bantock[1] has already ventured to call such as these parovarian cysts. There are certainly several facts which are hardly compatible with the theory of their ovarian origin: First, as pointed out by Dr. Bantock, the absence of Graafian follicles in the deeper part of the ovaries, and the unlikelihood of their developing in the direction of the greatest resistance; secondly, the curious fact that very few of the tumors, lying thus between the folds of the broad ligament, assume the common form of multilocular cysts, but they are almost invariably unilocular; thirdly, if they originate in the ovary they should be covered with a double layer of peritoneum, for Waldeyer has demonstrated that the ovary lies in a cup-shaped depression of this membrane, and an ovarian cyst must push the peritoneum forming this depression before it as it insinuates itself between the layers of the broad ligament and becomes invested by them. This double layer has not been noticed by others, and was certainly absent in my specimens. This subject needs thorough investigation.

RECENT PROGRESS IN PHYSIOLOGY.[2]

BY H. P. BOWDITCH, M. D.

MUSCULAR SENSE.

THE existence of sensitive nerves in muscles has been assumed by most physiologists as necessary to explain such phenomena as the feeling of muscular fatigue, the power of estimating the weight of objects, and the knowledge of the position of our limbs which enables us, consciously or unconsciously, to execute coördinated muscular movements. Although the sensitive nerves of the skin may aid to a certain extent in the performance of these functions, yet the part they play must be regarded as a very subordinate one. As conclusive evidence on this point may be mentioned the observation of Bernard[3] that the removal of the skin from the limbs of a frog does not essentially interfere with its powers of locomotion, while the section of the posterior roots of the sciatic plexus renders the animal's movements entirely uncontrolled and irregular (ataxia).

The principal opponent of the theory of muscular sensibility is Schiff,[4] who considers that our perception of the state of muscular contraction depends entirely upon a "central consciousness of innervation." In favor of this theory are the observations of Leyden[5] on ataxic and anæsthetic patients who still preserve the power of estimating the weight of objects. (Brown-Séquard's observations[1] that the channels of muscular sensibility do not decussate in the cord with the ordinary sensitive fibres are probably also to be interpreted as favoring this view.) Opposed to this hypothesis, on the other hand, are the experiments of Bernhardt,[2] who found that it made little or no difference in the accuracy with which weights could be estimated, whether the muscles were brought into contraction by an effort of the will or by electrical stimulation of the nerve. It seems probable, therefore, that the central consciousness of innervation plays an important though not an exclusive part in supplying information as to the condition of the muscles. The feeling of muscular fatigue depends, according to Schiff, not upon an affection of the sensitive nerves of muscles, but upon an interference with the circulation due to the limbs being kept too long in one position. In support of this view, Schiff makes the extraordinary statement that the same feeling of fatigue and pain which is experienced when the arm is held horizontally extended for fifteen minutes may be produced when the arm is *supported* in that position for the same length of time. Were this the case, we should, as has been well remarked, awake from sleep not refreshed and strengthened, but with violent pains in all our limbs. How the pain of muscular cramp is accounted for on this theory, Schiff does not explain.

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[1] Obstetrical Journal of Great Britain, i. 2, 1873.

[2] Concluded from page 65.

[3] Système nerveux, i. 251.

[4] Lehrbuch der Physiologie, i. 156.

[5] Über Muskelsinn und Ataxie. Virchow's Archiv, Bd. 47, S. 321.

Bernhardt also, in spite of his experiments above alluded to, is unwilling to admit the existence of nerves of muscular sense. He regards the estimation of weights as depending upon the central consciousness of innervation, the knowledge of the position of the limbs as derived through the Pacinian corpuscles, whose existence has been demonstrated on the articular nerves, and muscular pain (as in tetanus) as caused by pressure on nerves running through or over the muscles. This last hypothesis is of course quite inconsistent with the general law that the sensation caused by pressure on the trunk of a nerve is referred to its termination.

Anatomists as well as physiologists have brought forward reasons for attributing " sensitivity " to certain nerve-fibres distributed to muscles. Thus Bell, noticing that division of the facial nerve paralyzes the muscles of the face, although they also receive certain fibres from the trigeminus nerve, argued that certain sensations must have their origin in the muscles. Reichert,[3] in studying the terminations of nerves in the muscles of the frog, found a small number of fibres which ended very differently from the others, and suggested that they might be the sensitive muscular fibres. Kölliker[4] traced these nerve-fibres still farther,

[1] Archives de Physiologie, ii. 698.

[2] Zur Lehre vom Muskelsinn. Archiv für Phsychiatrie, 1872.

[3] Reichert and Du Bois Reymond's Archiv, 1851, page 29.

[4] Microskopische Anatomie, ii. 240.

and discovered their terminations in the fascia surrounding the muscle. Odenius was still more successful in following out the ultimate ramifications of these fibres. His account of them is essentially the same as the one which will be given below.

Thus, although weighty anatomical and physiological reasons could be given for assuming the existence of centripetal nerve-fibres in muscles, yet the absolute demonstration of the fact was until recently wanting. This gap in our knowledge of the subject has been filled by Carl Sachs,[1] from whose treatise on the subject most of the above citations have been taken.

The experiments of this investigator were as follows: The reflex convulsions of frogs poisoned with strychnia and picrotoxine were used as an index of the irritation of sensitive nerves. The muscles of one of the hind limbs were rendered motionless by division of the anterior roots on that side. The sartorius muscle of that side was then divided at its two extremities, and all its connections with the body, except the nerve, were severed. A thin plate of glass was then passed under it, and the preparation moistened with a drop of a three fourths per cent. solution of common salt. The muscle was then irritated by induced currents, and every contraction of the muscle was found to be accompanied by reflex convulsions of the rest of the body. It was thus shown that the sartorius muscle contains centripetal nerve-fibres whose irritation calls into activity the reflex centres of the spinal cord, but it was not shown that the contraction of the muscle, as such, is capable of stimulating the nerves in question, since in the above experiment the nerves themselves were irritated by the electrical current. It was therefore necessary to use some means of irritation which would act on the muscle and not on the nerve; ammonia is an irritant of this sort, and the sartorius muscle of the frog is particularly adapted to experiments of this kind, because its nervous distribution is limited to the central portion. The ammonia was applied by means of a piece of bibulous paper to a transverse section at one end of the muscle. A wave of contraction at once passed over the muscle, and reflex convulsions of the other limbs were immediately produced. It was thus shown that a contraction of a muscle is capable of stimulating centripetal nerves which are distributed through its substance and which enter the spinal cord by the posterior roots.

In order to obtain anatomical as well as physiological evidence of the existence of nerves of muscular sense, Waller's method of investigation was resorted to. This consists in dividing a nerve-root, or one of the component nerves of a nervous plexus, and studying the degeneration of the nerve-fibres which results from their separation from the nerve-centres. The course of the fibres thus divided can in this way be traced through the mixed nerve-trunks. If, for instance, six or eight weeks after division of the anterior roots of the nerves of a limb, it can be shown that the muscular nerves contain any undegenerated fibres, it may be assumed that these fibres are derived from the posterior roots. The application of this method to the sartorius muscle of frogs showed that the nerve supplying this muscle contains about twenty nerve-fibres, two of which only do not degenerate after section of the anterior roots. These two fibres may therefore be regarded as sensitive. (Schiff's[1] failure to find undegenerated fibres in the muscular nerves after section of the anterior roots is, according to Sachs, to be explained by the unsuitable methods of research adopted by that observer.) The converse experiment of dividing the posterior roots outside the ganglia did not give such definite results. The degeneration of the sensitive muscular nerves was found to be much slower and less complete than that of the motor nerves under similar circumstances.

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[1] Reichert and Du Bois Reymond's Archiv, 1874, pages 175, 491, 645.

Still further evidence of the difference between the two sorts of nerve-fibres is afforded by the result of local irritation applied to the nerves in their course amongst the muscular fibres. The irritations were produced by induced currents sent through very fine platinum electrodes, which were applied close to the intra-muscular nerve-fibres as seen under the microscope. Experimenting in this way, Sachs found that in most cases a very feeble irritation was sufficient to cause a contraction of those muscular fibres to which the nerve in question supplied terminal plates, while the rest of the muscle was unaffected. Certain nerve-fibres, however, were found to which the irritation had to be applied with considerably increased intensity in order to produce a muscular contraction, and the contraction when produced was not limited to certain muscular fibres but extended over all the neighboring contractile substance. This would seem to show that the nerves in question had no motor function, but that the contraction was due to a general stimulation, by side currents, of the muscles and nerves in the neighborhood.

Histological investigations by Sachs have also shown a decided difference in the mode of distribution of the two sorts of nerves. While the motor nerves, subdividing dichotomously, end after a short course in the so-called terminal plates, the sensitive nerves, ramifying often dendritically, pursue an extended and isolated course over a large number of muscular fibres, and give rise to delicate non-medullated nucleated fibrillæ, which frequently anastomose with each other, and terminate partly in the connective tissue around the muscles, partly in the interstitial connective tissue, and partly as very fine branches surrounding the muscular fibres themselves.

The author also discusses the nature of the process by which the sensitive nerves are irritated in the living muscle. The feeling of fatigue he regards as due to a chemical change in the substance of the exhausted muscle (formation of lactic acid, etc.). The perception of a muscular contraction is probably due to a mechanical irritation of the sensitive nerves which run between and around the muscular fibres, and therefore follow from necessity their changes of form.

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[1] Lehrbuch der Physiologie, i. 159.

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THE BRITISH MEDICAL ASSOCIATION.

The annual meeting of the British Medical Association will be held at Edinburgh in the month of August, beginning on Tuesday, the 3d, and lasting five days. The meetings of this association have been so successful during the last few years, and the character of the men who take a prominent part in its proceedings is of such high standard, that we should recommend to all those who have the welfare of our own association at heart a careful study of the British organization and of its method of conducting its meetings. Undoubtedly Great Britain possesses advantages over this country in its compactness, and the possibility of a general assembly of its physicians. The adoption of a medical journal as an organ of the association has been followed by a very large addition to the number of members. The price of the journal is included in the admission fee of one guinea. This in itself is no small inducement; but the chief advantage of such a journal is its organizing power, and the opportunity it offers to utilize the work done by the association and to put it in an attractive manner before the profession, — work that would otherwise be buried in transactions, as is the custom here.

Without recommending the adoption of a similar project by our own association, we think this feature offers a hint which might be taken advantage of. Articles should be read while the discussions of the annual meeting are fresh in the minds of the readers, and while the subjects treated of belong to the topics of the day, and not long after other questions have arisen to take their place. There are many ways in which this might be done. If judiciously managed it might produce a beneficial effect both upon our association and upon our medical literature. The annual addresses on important departments of medicine by leading men are an exceedingly attractive feature, and give a tone to the session which could not easily be acquired in any other way. The work of the sections does not appear to differ essentially from that of our own. We notice, however, that presidents of the various sections are selected with a good deal of care. For instance, this year, Dr. Quain, of London, is president of the section on medicine, Professor Lister of the surgical section, and Professor Burdon Sanderson of that on physiology. Officers are evidently selected for what they have done, and without reference to where they come from. The "side shows" of this annual entertainment are carefully attended to. The annual museum, as it is described, could be made a very valuable feature. The "conversazione," a peculiarly English institution, in spite of its name, combines entertainment with instruction. Finally, the excursions are arranged on a liberal and hospitable scale. The British Medical Association is undoubtedly the most successful organization of its kind that exists to-day. Would it not be well for the leaders of our own association, which they are now endeavoring to resuscitate, to study carefully the elements of this success?

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THE CITY SEWERS.

The second hearing before the sewerage commission, held on the 12th, although not bringing out any new facts, was simply confirmatory of the testimony given at the previous meeting, and was, in short, but a formal statement of a condition of things which has been universally acknowledged to exist for a far longer space of time than one would have thought possible without some serious outbreak of disease. As was stated in our last issue, the true condition of affairs, bad as it has been reported to be, is even now not realized to its fullest extent by the public. One would hardly believe that large sewers have been built up to a solid stone wall, and then allowed to remain years without any outlet whatever; that other sewers have been so arranged as to permit a free escape of sewer-gas into the streets, thus defeating one of the very objects for which they were constructed; that in some parts of the city there are no sewers at all, the houses draining into the surrounding soil. Indeed, the soil upon which the larger portion of the city stands is fairly saturated with sewage, which slowly oozes into a bank of dock mud which forms an almost complete circle around us. In addition to all this, numbers of watering-carts are daily washing the streets with water drawn from the very sewer-mouths, and tainted with the same dangerous material. Under the circumstances it is surely a matter of surprise that we have not worse odors than are now complained of.

The sewerage commission received its appointment many months ago. It has had time to gain a pretty thorough knowledge of the condition of affairs and the opportunities which exist for supplying a remedy. A member of the commission, the Secretary of the State Board of Heath, is already on his way to Europe for the purpose of making this matter a subject of special investigation. We trust that a study of the systems of Paris, of London, or of such a city as Hamburg, for instance, which possesses many conditions similar to those which exist here, will furnish the commission with valuable hints, and we hope that the autumn may not be allowed to pass away without the announcement of a plan which fairly meets the difficulty, is in no sense a compromise, and is fully worthy of the commission and the city. We have spent many millions in introducing water into the city, and are wont to pride ourselves upon its purity; let us take good care that it does not prove a curse instead of a blessing.

MEDICAL NOTES.

— Dr. Middleton Michel, in the *Charleston Medical Journal and Review*, in an article on the anatomy of the bullet-track and cicatrices of the wounds of entrance and exit, comes to the following conclusions: 1st. The bullet-track is not a conical wound. 2d. From the irregularity of the shot-track, parts of its walls are in contact, which soon adhere and heal, while suppuration elsewhere may continue for a time from both openings. 3d. In exceptional instances, the wound from a conical ball, accompanied by fractured bone and shortening of limb, heals spontaneously without suppuration. 4. The demolitionary effects of the minié ball on osseous tissue have been exaggerated, since the axial cleavage of the shafts of long bones was not so frequently encountered as has been generally stated. 5th. Misconception of the ravages of the conoidal ball led to unnecessary sacrifice of limbs. 6th. Differential features of entrance and exit wounds, if reliable, are the more readily seen the earlier these are examined. 7th. The cicatrices will often again indicate the separate orifices.

Dr. Michel, in a late number of the same journal, describes a modification of the operation for the removal of cancer of the lip. The operation is thus described: "Making a semicircular incision two lines from the diseased part, and entirely circumscribing it, the whole growth was dissected away down to the mucous layer, this being preserved intact, then paring away the remains of all suspicious tissue with the scissors, and saving thereby every available portion of integument, which the usual V-incision could not have done." In cases where the cancer involves the whole lower lip, he recommends preservation of the mucous surface, and allowing the wound to remain open and heal by granulation, after the manner of Richeraud, by which a most satisfactory result is said to be obtained.

— Professor Billroth's case of extirpation of the larynx has been followed by a second similar operation, performed by Professor Bottini, of Novara. The patient was a healthy young man, who, on account of dyspnœa, had already had laryngotomy and galvanic cauterization of the larynx performed. The operation lasted ninety minutes, and left the patient much exhausted. He was, however, revived by injection of beef tea and wine. It was found that the larynx was quite obstructed by a grayish-red tumor. In spite of an attack of erysipelas and a series of abscesses, the man gradually improved, regained the power of deglutition, and was able to cough, breathe, and sleep well. The wound has healed up, and the case, save future accidents, may be looked upon as successful.

— Drs. Kaposi, Auspitz, and Neumann have been appointed extraordinary professors of dermatology and syphilis in the University of Vienna. They have hitherto held the position of *privat-docent*. Dr. Carl Störk, *privat docent*, has been appointed extraordinary professor of laryngoscopy in the same university.

— Mr. Seymour Haden's perishable coffins lately exhibited in England are described in the English papers. The coffins were simply wicker baskets, of the ordinary coffin shape and of various sizes. Two of them had their meshes filled by moss, but the rest, which were supplied by Mr. Kirby, a basket maker at Derby, were left with their meshes open, like those of an ordinary waste-paper basket. Two or three were double, with a space two inches or three inches in width between the inner and the outer basket; and this space is intended to be filled with charcoal, for cases in which any precaution against infection and decomposition may be required. The following account is given of them: "It is necessary, perhaps, to explain that the models shown are merely suggestive, and that the majority of them do not as yet fulfill all the conditions essential to their practical use. 1. The mesh in most of them should be larger than it is, and as open as is consistent with strength and the perfect retention of their contents, which contents, again, should consist of the larger ferns, mosses, lichens, herbs, fragrant shrubs, and any of the coniferæ, willows, or evergreens, which are always to be had. 2. The osiers composing the baskets should be light (two thin ones being better than one thick one), and no more solid wood should enter into their construction than is necessary to preserve their form. 3. They should be of white or stained willow, without varnish or other preservative covering. 4. Accompanying each of them should be a narrow leaden band or ribbon, pierced with name and date of death, to be passed round the chest and lower limbs, and through the sides and over the top of the basket: (i) for retaining the body in its position; (ii) for the subsequent identification of the bones; (iii) for sealing the coffin, as a guarantee that the contents have not been disturbed. 5. In special cases linings of some imperishable material for a few inches upwards from the bottom will be necessary; and, in other cases, such modifications of the ordinary form as may insure a complete inclusion of the body in wool, charcoal, or other disinfectants. 6. Other materials which are light, strong, perishable, inexpensive, adapted for carriage, and favorable to the dissolution of the body may do as well and possibly better than these wicker baskets. Readiness of carriage and the insurance of resolution being the main objects aimed at, several such materials do, in fact, suggest themselves, and may afterwards come to be employed."

LETTER FROM PHILADELPHIA.

Messrs. Editors, — The Centennial Medical Association, which originated in the Philadelphia County Medical Society, promises to be a very important organization. Arrangements have been made to hold an International Medical Congress early in September, 1876, in Philadelphia, at which papers upon medicine and medical progress will be read. The congress will consist of delegates, native and foreign, representing the American Medical Association, various State medical societies, and the medical societies of Europe, the British Dominions, Central and South America, Sandwich Islands, both the Indies, China, and Japan. It has been agreed that no vote shall be taken during the sittings of the congress upon any topic whatever. The preparation of the discourses has been intrusted to able hands, and they will be published in an appropriate commemorative volume. A circular, stating the object of the Centennial Medical Commission, has been prepared, and will be sent, by means of our foreign ministers and consuls, to the proper bodies. Special invitations to medical gentlemen of high scientific and social position will be issued, and a great object will be to make the meeting truly representative of the profession, both in its native and in its foreign elements.

To this end, and to prevent the admission of any unworthy applicants, proper testimonials will be required by the committee on credentials. The choice of Professor S. D. Gross as president of the association gives universal satisfaction. A more distinguished or dignified representative of American physicians could hardly have been found.

The trustees of Jefferson Medical College have purchased real estate in the rear of the present collegiate buildings, and will proceed to erect a hospital of useful dimensions. The hospital heretofore used by the college is limited in size, and merely a convenience in cases incapable of being moved, but too contracted to admit of clinical uses on behalf of students. The new hospital, which will probably be roofed in by September, will greatly increase the usefulness and attractiveness of the school.

The late John H. Towne, of Philadelphia, in his will made a large addition to the endowment fund of the University of Pennsylvania. After making liberal provisions for his family and relatives, he bequeathed $10,000 to the University Hospital, and after various other bequests to the Pennsylvania Academy of Fine Arts, to the Academy of Natural Sciences, etc., he constitutes the trustees of the University of Pennsylvania residuary legatees of the remainder of the estate — probably $300,000. The university has also a reversionary interest in the estate, which may ultimately make the total bequest upwards of one million dollars. The money thus bequeathed is to be held as a portion of the endowment fund of the university, and the income is to be applied solely to the payment of salaries of professors and other teachers in the department of science, — a most wise provision.

The physiological laboratory of the University Medical School was recently opened, and is now conducted by Dr. Ott, who received his training in Germany. The laboratories for chemical and histological work are also now open to students who may wish to pursue these branches of study.

The summer medical schools, both of the University and Jefferson, are attended by classes numbering, perhaps, fifteen per cent. of the winter complement. The regular courses of lectures terminate with the winter sessions, and lectures upon pathological anatomy, toxicology, histology, laryngoscopy, physical diagnosis, syphilis, urinary chemistry, and minor surgery are now being given by the sub-faculties. There are also surgical, medical, skin, and eye clinics. Attendance upon these lectures and clinics is not obligatory; consequently, the number of students at the various lectures varies with their interest in the subjects presented. Lectures are over at two P. M., and the remainder of the day is given to dissection and reading. The effect of the Philadelphia climate at this season being somewhat enervating, students (with the exception of the few earnest, far-seeing ones), without especially cultivating the Italian language, learn, I fancy, to the full, the meanings of *dolce far niente.*

In the matter of the objections made by the committee of citizens (medical and lay) to the construction of the abattoir on the Schuylkill in West Philadelphia, President Judge Alison recently gave the opinion of the court to the effect that "the objections related chiefly to the plans for filtering and sewerage, and that after carefully considering these plans and the opposition to them, the court was unable to say that they were defective. They would have to be left to the result of practical tests. They were, of course, subject to the risk of being adjudged nuisances, if upon actual use that character became attached to them. The control of the court would be exercised over them, but the work will be allowed to go on." As to the question of storing hogs and cattle, the judge said the court had decided the case simply as one involving the establishment of a slaughtering place or abattoir, and had never considered the matter of an entrepôt or storage place for animals, and hence had no opinion to give. Of course, if any trouble from this cause should hereafter arise, plaintiffs would be at liberty to appear and ask for relief. The place would be closely watched. So the Pennsylvania Railroad Company will have their way, as I anticipated in a former letter. It remains to be seen whether the death-rate of Philadelphia will be increased by this supplement to her other nuisances.

Professor Da Costa, twelve months ago, delivered a lecture upon "Strain and Over-Action of the Heart," in the Toner course at Washington. It was an instructive and suggestive discourse, and has since been published. I am glad to learn that its excellence led a professor in the University of Zurich to translate it into German.

The new pavilion ward of the Presbyterian Hospital was the other day opened to the public. I gave you an accurate description of the ward in a former letter. It supplies a much needed want, and offers facilities for the treatment of surgical cases that are possessed by few hospitals. The pavilion cost $14,000.

A pleasant incident which occurred during the recent meeting of the American Medical Association at Louisville was the presentation to Professor Gross, by the physicians of that city, of a pair of very handsome, thorough-bred horses. Professor Gross practiced and taught in Louisville for many years before he was called to the Jefferson Medical College of this city.

Dr. W. W. Keen recently performed ovariotomy upon a woman aged forty-six, who began to menstruate at fifteen, ceased at forty-four; was always regular; married at nineteen; bore only one child, and never had any uterine trouble. The abnormal growth first made itself known about thirty months ago. Last October the attending physician removed five gallons of greenish, viscid fluid from the abdominal cyst, after which a slight peritonitis set in, but the patient was more comfortable than formerly. Later, she came into the hands of Dr. Keen. A thin, spare woman, measuring six inches from ensiform cartilage to umbilicus, nine inches from this point to the pubes; greatest girth thirty-three inches. The swelling was almost wholly below the navel, with fluctuation on palpation. Below the fluid a solid body was felt. There were two adhesions about three inches square. One surrounded the site of the tapping. The other was below it, and corresponded to a hard mass. Ascitic fluid flowed around these adhesions, and revealed them distinctly. The mass was movable only with difficulty. Palpation showed the upper part of the tumor to be cystic, the lower portion to be solid. Extending from the pubes and obliquely to the right was a mass resembling an elongated uterus. Examination, per vaginam, revealed an os and cervix nearly normal as to size, position, and relations. The tumor was felt behind the uterus, but a distinct depression existed, and could be felt between them. The sound entered the uterine cavity two and one half inches, upward and somewhat to the right. The uterus was not movable independently of the tumor, movement of the latter communicating itself to the uterus. Per rectum the tumor was easily felt, and gave the impression that its pedicle was very short.

May 20th. Dr. Keen aspirated a small quantity of fluid from the cyst for examination. The fluid was purulent, and contained compound, granular corpuscles, blood and pus cells, but was composed mainly of distinct ovarian cells.

The woman was gradually sinking, and the general discomfort was so great that her life had become a burden; therefore, May 22d, Dr. Keen removed the growth. After tapping the main cyst, and finding the lower mass too solid to admit of further diminution in size, he extended the incision from the umbilicus six and one half inches downwards, or nearly to the pubes, before the tumor could be removed. It was found to be slightly adherent to the parietes, omentum, and intestines, and, having no pedicle, the growth hid everything beneath it. Dr. Keen consequently tied a strong ligature around its base, and removed the larger portion of the tumor by cutting directly through its substance. The base was discovered to be attached directly to the uterus, rectum, and left side of the pelvis. The remainder of the growth was removed. Three ligatures were passed around the broad attachments of the tumor, thus securing several large vessels which were troublesome. Eight ordinary ligatures were required to arrest hæmorrhage from smaller vessels. The attachments were touched with Monsel's solution; the eight small ligatures were cut short off, the three larger ones brought out of the cavity and left. The wound was closed by eight silver sutures.

In its upper portion the tumor consisted of a large cyst, containing four pints of fluid and two or three smaller cysts. The inner surface of the large cyst was coated with soft, papillary growths, numerous detached masses of which formed a sediment in the fluid. The lower portion was a hard, solid mass, divided by distinct septa of connective tissue into smaller masses, which were whitish in color, and subdivided into masses still smaller in size.

In spite of the most nourishing and careful treatment the patient gradually sank, and died the day following the operation.

The autopsy discovered the intestines glued together with lymph of recent formation. Below the umbilicus the omentum was attached to the abdominal wall transversely, dividing the main peritoneal cavity into two parts. Above this level were the remains of old inflammatory effects. No hæmorrhage had occurred after the operation. The tumor had been attached to nearly the whole of the left half of the uterus, laterally and posteriorly, and to the rectum in Douglas's cul-de-sac. It was from the rectal attachments, as well as from the attachments to the left broad ligament, which was the pelvic support of the tumor, that the annoying hæmorrhage had taken place.

Microscopic examination by Dr. Joseph G. Richardson made it evident that the growth was "a rather hard variety of encephaloid or medullary carcinoma." It was thought singular that the ante-mortem examination of the cystic fluid revealed no sign of the true nature of the tumor.

PHILADELPHIA, *June* 15, 1875.

DR. ESTES HOWE.

MESSRS. EDITORS, — In preparing the article upon the Medical Profession of Massachusetts during the Revolutionary War, I endeavored to give sketches of all those surgeons who were connected with the American army; and for this purpose I consulted such biographical works as have been accepted as authority on this matter. I doubt not that I may have omitted some who were useful in the army, and yet failed to attract the attention of the biographers. One such I have already discovered — a valuable officer, a physician of high standing both in civil and military life, and a citizen whose memory is still cherished in that section of the State where he resided; and I have secured the following sketch, which should justly be added to the original article.

Dr. Estes Howe was born June 24, 1747, in Belchertown, Mass. His father, Samuel Howe, was one of the earliest settlers of that town, having emigrated to what was then called the "West," from Marlboro; he was a captain in the French war, from 1755 to 1763, and at Lake George. He was also a member of the first and second Provincial Congresses.

Dr. Howe, who, while yet a boy, attended his father on one of his campaigns, as a drummer, studied his profession and commenced practice in his native town, where he married on the 18th of February, 1773, Susanna, daughter of Captain Nathaniel Dwight. On the 25th of April, 1775, six days after the battle of Lexington, he joined the American army, was commissioned as surgeon, and was attached to the regiment of Colonel David Powers. This regiment was engaged at Bunker Hill, and was afterwards stationed at Roxbury. On the 1st of January, 1776, he resigned and returned to his practice for a year, after which he received a second commission, as surgeon of the regiment commanded by Colonel Rufus Putnam, with which he remained until May 1, 1779, when he finally left the army. This regiment was stationed at Saratoga many months, during which time he conceived a high admiration of General Gates, for whom he named a son.

Having resumed his practice at Belchertown, he continued there during the remainder of his life, dying March 3, 1826. His professional life extended over half a century, during which he secured the esteem and respect of the community in which he lived. Like many of the physicians of that day, he was not the possessor of a diploma, but he was an early member of the Massachusetts Medical Society. Setting high value on classical culture, he educated two of his sons at Dartmouth College, and one at Williams. He left a record worthy of admiration, and a name which is a valuable addition to the list of patriotic physicians who did so much to carry our country through the war of the Revolution. GEORGE B. LORING.

SALEM, *July* 15, 1875.

WEEKLY BULLETIN OF PREVALENT DISEASES.

The following is a bulletin of the diseases prevalent in Massachusetts during the week ending July 17, 1875, compiled under the authority of the State Board of Health from the returns of physicians representing all sections of the State: —

We have to note again the very general prevalence of intestinal disorders. The reports show an increase of these diseases in nearly all sections of the State. The summary for each district is as follows: —

Berkshire: Diarrhœa, cholera morbus, whooping-cough. Pittsfield reports a fatal case of diphtheria.

Valley: Diarrhœa, cholera morbus, rheumatism.

Midland: Diarrhœa, rheumatism, cholera morbus, cholera infantum, dysentery.

Northeastern: Diarrhœa, cholera morbus, cholera infantum. Less sickness than in other parts of the State.

Metropolitan: Diarrhœa, cholera morbus, cholera infantum, scarlatina, rheumatism. Typhoid is increasing. Brighton and Melrose report diphtheria of a severe type.

Southeastern: Diarrhœa, rheumatism, cholera morbus, dysentery, cholera infantum. Towns on Cape Cod and in Dukes County report the measles as quite prevalent in that section.

Comparing the present week with the last we find that diarrhœa, cholera morbus, cholera infantum, dysentery, and typhoid fever have increased in prevalence; while bronchitis, measles, diphtheria, rheumatism, scarlatina, whooping-cough, and pneumonia are disappearing.

F. W. Draper, M. D., Registrar.

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COMPARATIVE MORTALITY-RATES FOR THE WEEK ENDING JULY 10, 1875.

	Estimated Population.	Total Mortality for the Week.	Annual Death-rate per 1000 during Week.
New York	1,040,000	743	37
Philadelphia	775,000	412	28
Brooklyn	450,000		
Boston	350,000	147	22
Cincinnati	260,000	150	30
Providence	100,700	29	15
Worcester	50,000	16	17
Lowell	50,000	24	25
Cambridge	50,000	12	12
Fall River	45,000	27	31
Lawrence	33,000	12	19
Springfield	33,000	7	11
Lynn	28,000	7	13
Salem	26,000	8	16

Military Appointment. — Dr. W. F. Southard, of Baldwinsville, reappointed Surgeon of the Second Corps of Cadets, passed a successful examination before the Board of Medical Officers, M. V. M., July 2, 1875.

Edward J. Forster,
Surgeon Fifth Regiment of Infantry, M. V. M., Recorder of Board.